

LETTERS TO THE EDITOR.

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The National Antarctic Expedition.

I HAVE recently been made acquainted with certain hypotheses which are believed to explain the motives which induced Prof. Gregory to resign the position of scientific director of the National Antarctic Expedition. Thus, it is commonly believed that he was influenced by his family and friends. Indeed, the opinion has recently been expressed that I was, perhaps, the cause of his withdrawal, or that, at least, I advised it. It is impossible to imagine how such an opinion can have arisen if my letter to the Fellows of the Royal Society had been read with any attention, unless, indeed, I have failed to give a fair and accurate account, in spite of most serious efforts, put forth with a grave sense of responsibility.

I am, however, now able to set the matter at rest by a quotation from Prof. Gregory's letters received since the circulation of my account of the negotiations. I am quite sure that Prof. Gregory would have no objection to this use of his words in order to confront the unfounded rumours which have obtained currency.

It may be remembered that after the meeting of the Joint Committee on March 5, at which Major Darwin's proposed changes in the conditions offered to, and accepted by, Prof. Gregory were approved, although I had strongly opposed the introduction of any alteration whatever, "I wrote to Prof. Gregory a full account of what had happened, carefully explaining that his representative and many of his friends supported the changes, that I had confidence that the proposal was made to enable the Geographical Society to accept the instructions, and that it was not intended to prevent, and, I believed, would not prevent, his being landed" (p. 6 of my letter).

I have now received two letters from Prof. Gregory, one written on April 16, before he had received mine, the other on April 23, after he had received it.

In the former he says: "I hear that the Joint Committee has accepted some of Darwin's amendments; but as I do not know what they were I can form no opinion. But —, —, and — say they make no difference. I hope not."

In the latter, written in reply to my letter, he says: "Very many thanks for your fight against Darwin's amendment, which I should not have accepted had I been in London or been advised of it by cable. However, I suppose it is now too late to go back on it; and as it has [been] accepted for me I must trust to luck."

Later on in his letter the explanation of his resignation becomes perfectly clear; indeed, he asks me to make it known. In the event of the President of the Geographical Society declining to sign the instructions, he says: "Please let it be known that, except for a modification backward of Darwin's amendment, I will not accept another change."

Between my letter describing the meeting on March 5 and May 15, when his final resignation was known, I held no communication of any kind with him. But others had communicated those further changes which he was determined not to accept.

It must be clear to any one who will read the history of the negotiations carefully, that he thought, and had good reason to think, that he was being trifled with, and felt that the time had come—to a less patient man it would have come long before—when he would no longer submit to the vigorous attacks of the Royal Geographical Society and the weak, half-hearted defence of the Royal Society.

A few hours after the above words were written a letter arrived from Prof. Gregory dated May 5, just after he had received the cable from the new Committee of six. The letter indicates clearly the reasons which induced him to withdraw, and I therefore quote several passages from it. The letter was written hurriedly, and not intended for publication; but I know that Prof. Gregory would assent to my action, pursued as it is with the object of preventing the misinterpretation of his motives. A few unimportant verbal changes have been made.

"You at least," he says, "will not have expected me to accept the cabled terms. I was not surprised at them; only surprised that the Royal Society had given way apparently so readily and that I heard the result a month earlier than I expected."

"The terms proposed appear to me, as far as I understand them from the cable, a complete surrender of what the Royal Society's representatives declared in February was essential to the proper execution of the magnetic work. The position gives no power to secure a fair opportunity for work to the man who would have to bear the blame for scientific failure."

"To accept responsibility without adequate power is a false position which is almost sure to lead to trouble. No man has a right to take such a position. As I do not think the powers are adequate to the responsibilities, it is my simple duty to withdraw. I hope the Royal Society will find a better man, who will be satisfied that he can make the Expedition a scientific success on the instructions given. I am not; therefore I must withdraw my provisional acceptance of the appointment."

"It will be difficult to prevent my withdrawal being misinterpreted. I had thought of cabling to ask you to publish an explanation, but thought it best to leave you to act as you thought best. I can absolutely rely on your judgment, and know you will have done anything necessary to repel insinuations."

I have done my best to prevent Prof. Gregory's motives from being misunderstood, and it is with the same object that this communication is now written and accompanied by quotations from his letters.

He concludes with a reference, which is far too appreciative, to the support which—unfortunately for the scientific prospects of the expedition and, I must add, unfortunately for the credit of the Royal Society as the guardian of the interests of science—received, at the later stages of the negotiations, the help of so small a proportion of my colleagues.

Oxford, June 11.

EDWARD B. POULTON.

A Raid upon Wild Flowers.

Prof. L. C. MIALl, in the last number of NATURE, makes very definite and serious charges against the organisers of the vacation course for Essex teachers in the New Forest. As author of the programme so severely, and, as I contend, unfairly, criticised by your correspondent, I should be glad to be allowed an opportunity for reply.

The programme, as you will see by the copy enclosed, consists of two parts, the first dealing with a series of Saturday afternoon botanical rambles in our own county and the other with the proposed vacation course to be held at the New Forest. The first is of a pioneer character, and is open to all teachers whether they are familiar with botany or not, while the vacation course is organised for those of our teacher-students who have already received one, two or more years' instruction in laboratory and field-work in botany at the central institution here. For this course special application must be made to the committee.

From a perusal of the programme Prof. Miall accuses the Committee for Technical Instruction in Essex with organising a raid in the New Forest especially upon wild flowers tending to extinction, and bases his charge upon certain alleged facts. Your readers are told that with respect to these rare plants our intention is to collect, &c., "not only single specimens but duplicates for special fascicles." There is no such reference in the programme of the vacation course in the New Forest, but in a note at the end of the Saturday afternoon programme occurs these words.

"Opportunity might be taken, during the course of the Saturday rambles, of commencing a school herbarium, or collection of dried plants illustrative of the flora of collector's own district. A type collection would naturally be arranged in botanical order, but duplicates might be used for special fascicles representing, for example, 'meadow plants,' 'cornfield weeds,' &c."

The letter continues—"Local guides are to direct them to the last retreats of the rare plants of the New Forest." This, too, is a mistake. In the Saturday afternoon rambles we are to be accompanied by local guides whose names and addresses are given in the programme, but no such arrangements were made for the New Forest. It is true that I sought the sympathy of local naturalists, and, indeed, so anxious was I to prevent even the suspicion of "raiding" that I wrote to the Rev. J. E. Kelsall, the local representative of the Selborne Society, whose strong views on the preservation of the plant and bird life of the New Forest are so well known, to tell him of our proposal and to assure him that our chief object was the study of living plants, and that if we discovered anything rare, or even scarce, it would be left untouched by our students; and I thought that

the publication of the fact that Mr. Kelsall and Mr. Dale, secretary of the Hants Field Club, might be able "to accompany the party on one or more of its rambles" a sufficient guarantee that the rights of wild plants would be respected.

Furthermore, on the title page of the vacation course programme, p. 9, and printed in conspicuous black type, is the following notice: "Members of the party will, of course, refrain from uprooting rare or scarce specimens." Yet Prof. Miall alleges "there was no such restriction in the printed programme"!

In the daily itinerary as printed in the programme reference is made to the character of the scenery, the soil and surface geology, the prevailing vegetation, and to some of the rare plants growing in the neighbourhood. From what we have already shown it could hardly be our intention to raid these rare plants, and especially as several of those mentioned will be out of flower in August. Indeed, so particular are we in these rambles that the needless uprooting even of the commonest weed is discountenanced, as may be seen in the further notice on p. 3 of the programme.

In comparison with such a particularly odious charge as plant extermination, the other strictures of your correspondent's letter are, of course, scarcely worth noticing; yet even with respect to these I cannot resist pointing out that Prof. Miall's statements are strangely at variance with the actual facts. For example, he writes: "It is enough to condemn the programme as an educational project that novices knowing little or nothing of field-botany are set to study the subspecies of brambles." But does the programme so recommend? It distinctly says in reference to this (p. 18), that "their identification will give capital exercise in critical observation to the more advanced worker."

The real object of these field-studies, as stated on the front page of our programme, is to give teachers "an insight into the way in which plants grow, especially in their relations with their environment—the influence of external conditions, such as light, heat and moisture, upon their form, the mutual relationships between plants and animals and the influence of one organism upon another," and is in no way connected with collecting in the sense used by Prof. Miall. The vacation students have varied interests—flowering plants, algæ, leaf-fungi, &c., and the evenings are to be spent in discussing "the most interesting of the objects collected" and on the "preservation" of such as may be useful for class-work in the winter courses. Readers of NATURE will understand that work of this sort does not mean the collection of rare flowering plants.

Perhaps because of the peculiar gravity of the charge I may, in conclusion, be allowed to introduce one personal note into the reply. I should like to say that although I have conducted field studies in botany for the last twelve years (including two summer courses at the New Forest), yet, as it happens, I am no collector myself, and have never made what botanists would call a collection of dried plants in my life. Furthermore, I have never possessed, or even "coveted," a single specimen of a rare British plant. On the contrary, my sympathies are, of course, entirely with those who are opposed to any interference with our native flora, and I do most strongly protest against this attempt of Prof. Miall to connect in any way whatever our botanical work with such objectionable practices.

I should be glad to send a copy of the programme to any one who may care to see it.

DAVID HOUSTON.

County Technical Laboratories, Chelmsford, June 10.

Emanations from Radio-active Substances.

IN a recent number of the *Comptes rendus* of the Paris Academy (March 25) an account appeared by MM. P. Curie and A. Debierne of the production of a radio-active gas from radium. In their experiments some radium was placed in a glass vessel and the air exhausted by means of a mercury pump. It was found that the vacuum steadily decreased, due to the giving off of a gaseous substance from the radium. A small amount of the gas thus collected was found to be strongly radio-active. It caused phosphorescence in the glass tubes over which it passed, and in course of time blackened them. Substances exposed in the gas became themselves temporarily radio-active.

Some time ago (*Phil. Mag.*, January and February 1900) I showed that thorium compounds continuously emitted radio-

active particles of some kind, which preserved their radio-activity for several minutes. This emanation possessed the remarkable property of causing all bodies, in contact with it, to become themselves radio-active. In an electric field the excited radio-activity could be concentrated and confined to the negative electrode. In this way I was able to make a fine platinum wire become a very powerful source of radiation.

The excited radio-activity gradually diminished, falling to half its value in about twelve hours. The specimen of impure radium then in my possession gave out no emanation and caused no excited radio-activity. Later, Dorn, using the same methods, showed that a preparation of radium from P. de Haen, Hanover, gave out an emanation similar in properties to thorium. With a specimen of radium obtained from the same source I have found that the emanation given off is small at atmospheric temperature, but can be enormously increased by slightly heating the radium. In this way I have obtained ten thousand times the amount of emanation given off at ordinary temperatures. An account of these experiments is given in the *Physikalische Zeitschrift* (April 20).

By passing the emanation with a current of air into a closed vessel, and then closing the openings, the emanation remains radio-active for a long time. The radio-activity decreases slowly, but is still quite appreciable after an interval of one month. M. and Mme. Curies, some time ago, stated that they had obtained a radio-active gas which preserved its activity for several weeks; this is possibly identical with the emanation.

Up to this point I had been unable to obtain any definite evidence whether the so-called emanations were vapours of the radio-active substances, radio-active gases, or radiating particles large compared with a molecule. The radium and thorium, when placed in an exhausted tube, gave no appreciable lowering of the vacuum, and no new spectral lines could be observed. The quantity of substance emitted was too small to examine by chemical methods.

Quite recently, however, some light has been thrown on the question of the nature of these emanations by examining their rate of diffusion by an electrical method. In these experiments I have been assisted by Miss H. T. Brooks, and the results point to the conclusion that the emanation from radium is in reality a radio-active gas, with a molecular weight probably lying between 40 and 100.

There is one distinct feature which distinguishes the emanations from radium and thorium. The thorium emanation loses its radio-activity in a few minutes, while the excited radio-activity due to it lasts several days. The radium emanation, on the other hand, preserved its radiating power for several weeks, while the excited radio-activity due to it disappears in a few hours. In the following experiments it was only possible to experiment with radium emanation, on account of the rapid decay of radio-activity of the thorium emanation.

The diffusion apparatus was similar to that which had been employed by Loschmidt in 1870 in his determinations of the coefficients of interdiffusion of gases.

A brass cylinder, 73 cm. long, 6 cm. in diameter, was divided into two equal parts by a metal slide, which could be opened or closed. The ends were closed by insulating ebonite stoppers, through which passed central rods half the length of the tube. In order to introduce the emanation into one half of the cylinder the slide was closed, and a slow current of air, which had passed over slightly heated radium and thus carried the emanation with it, was passed through the cylinder. When a sufficient amount had been introduced the current of air was stopped and the openings closed. After standing for an hour or more the slide was opened, and the radio-active emanation slowly diffused into the other half of the cylinder. The amount of emanation in each half of the cylinder after any interval was tested by observing the current through the gas, when a suitable P.D. was applied, by means of an electrometer. The current is carried by the gaseous ions which are continually produced by the radiation from the emanation. From these observations the coefficient of inter-diffusion of the emanation into air at atmospheric pressure and temperature can be readily deduced. The experiments are, however, complicated by the excited radio-activity on the electrodes, which must be taken into consideration.

So far as the observations have gone up to the present, the coefficient of diffusion of the emanation into air has a value between 0.10 and 0.15, and probably nearer the former. Now the coefficients of inter-diffusion of some known gases and vapours